PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

March 3, 1877.

XIII. *Note on the Polarization of Heat.*

By G. Carey Foster, *F.R.S.*

The following determinations of the amounts of heat transmitted by two Nicol's prisms, whose principal sections make different angles with each other, were recently made in the Physical Laboratory of University College, London, by Mr. M. J. Jackson. Although the results amount to nothing more than an additional verification of a relation that is already thoroughly established, I venture to put them on record, not only because such verifications are satisfactory in themselves, but also because the apparatus required for conveniently repeating experiments of this kind is not always at hand.

The source of heat was a rather powerful paraffin-oil lamp (supplied by White, of Glasgow, for use with a Thomson's quadrant electrometer). The rays from the lamp were concentrated by a lens of 7·5 centims. diameter and about 22 centims. focal length, placed so as to produce a real image of the lamp-flame within the silvered reflecting cone of the thermopile. Immediately behind the lens (on the side next the lamp) a double screen of polished sheet brass was placed, whereby the radiation could be cut off or allowed to pass at will. On the other side of the lens came two Nicol's prisms, each about 20 centims. long, and giving a clear circular field about 6·7 centims. in diameter. The prisms are protected at the ends by disks of thin glass, which were left on during the experiments. It is to the possession of these fine prisms, made for me by Mr. C. D. Ahrens, that the possibility of making the experiments with so much ease was due. The thermopile, which was about 95 centims. distant from the lens, was protected from stray radiation by a double hood of tin-plate. The galvanometer was a reflecting instrument of low resistance, on Sir William Thomson's principle, made by my assistant, Mr. Grant. By means of a commutator inserted between the thermopile and the galvanometer, two opposite deflections were obtained for each position of the prisms. In the following Table, the column headed δ_1 gives the means of the deflections to right and left when the angle, θ, between the principal sections of the prisms was measured in one direction; and the column headed δ_2, the corresponding deflections when this angle was measured in the opposite direction. The numbers denote divisions of the galvanometer-scale.

Angle between principal sections $=\theta$.	Deflections.			$\frac{\delta}{\cos^2\theta}$.	δ' (calculated).
	δ_1.	δ_2.	Mean δ.		
0°	32·25	31·5	31·9	31·9	31·1
15	29·75	29·25	29·5	31·6	29·0
30	22·75	23·5	23·1	30·8	23·3
45	14·75	14·75	14·75	29·5	15·5
60	7·75	7·75	7·75	31·0	7·8
75	2·25	2·0	2·1	31·7	2·1
90	0·5	0·25	0·4	......	0·0
			Mean ...	31·1	

The numbers in the last column of the Table are calculated by the formula $\delta' = 31{\cdot}1 \cos^2\theta$. It will be seen that the observed mean values (δ) never differ from the corresponding calculated values (δ') by a whole division of the scale, which represents about as high a degree of accuracy as can be expected from the method of observation employed, it being impossible to read with certainty to any thing less than half a division: where quarter divisions occur in the Table, they result from taking the means of positive and negative deflections.

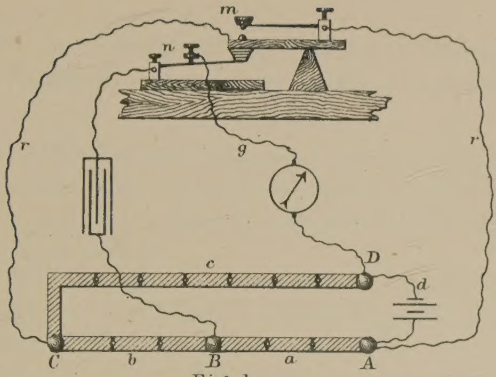


Fig. 1.

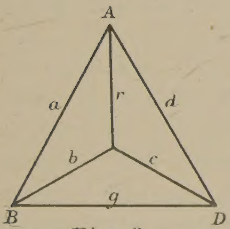


Fig. 2.

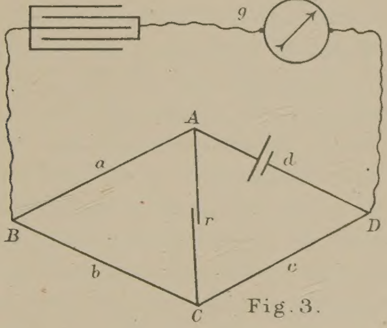


Fig. 3.

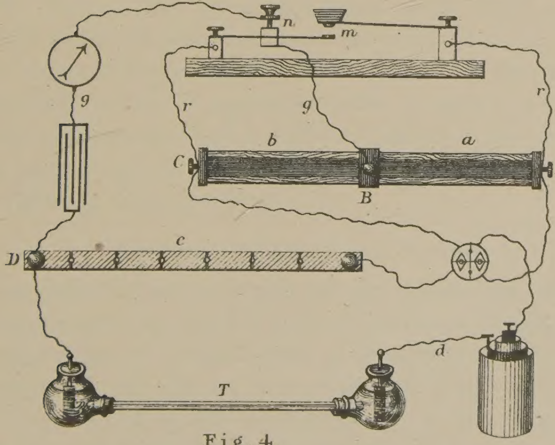


Fig. 4.

XIV. *On a Modification of* Mance's *Method of measuring Battery Resistance.* *By* Oliver J. Lodge, *B.Sc.*

[Plate II.]

The modification here suggested consists simply in using a galvanometer and condenser instead of a galvanometer alone, so as to detect variations in difference of potential instead of variations in current.

By this change it is converted into a strictly *null* method. Moreover it is now possible entirely to get rid of the effects of variations in the electromotive force of the battery, which are very annoying in any of the ordinary methods and prevent accurate measure. This is accomplished by breaking the galvanometer-circuit the instant after the battery is short-circuited. Fig. 1 (Pl. II.) is a diagram of the connexions for measuring the resistance of the battery d, with the keys shown on a large scale: m partially short-circuits the battery when depressed; n closes the galvanometer and condenser circuit *unless* depressed. The two keys are electrically independent; but the stand of the upper one is balanced so as to rest partly on the spring of the lower one (which must be strong). On depressing the upper key, the first effect is to close the circuit marked r at the point m; the second, and immediately succeeding, effect is to break the circuit marked g at the point n. The same object would be accomplished more conveniently by a single double-contact key made on purpose, as shown in fig. 4. The object of the double key is fully explained below. A B C D represents a box of resistance-coils; a and b are large and equal resistances; and c will be equal to d, the resistance of the battery, whenever the galvanometer-needle is unaffected by pressing down the keys.

Resistance-measurements in general.

Consider the arrangement of six conductors joining four points (commonly known as the Wheatstone's bridge) as forming the edges of a tetrahedron or triangular pyramid (fig. 2). It is obvious, (1) that, as far as position is concerned, every conductor has precisely the same properties as any other, and (2) that any one conductor is adjacent to four of the others and opposite to the remaining one. Call the resistances of pairs of opposite ones a and c, b and d, r and g, and let electromotive forces be caused to act in any manner through any of them; then it can be shown that when $ac=bd$, r and g are "conjugate conductors," or that variations in the conductor r have no effect whatever on the current in g, and *vice versâ*, no matter whether these variations are simple changes of resistance or the introduction of new electromotive force.

By ascertaining, then, whether the insertion or removal of batteries at r has any effect on a galvanometer at g, one can observe whether the relation $ac=bd$ is or is not fulfilled, and can change one of these resistances until it is. For the case when a, b, c, and d are simple metallic conductors, this is Wheatstone's method of comparing their resistances.

Again, reciprocally, when this relation is fulfilled, no change in g can affect the current through the battery in r; and therefore, if this battery in r is the only electromotive force in action, a change in the resistance of g does not affect the current at all anywhere. Hence a galvanometer in, say, d will show a constant deflection whether the resistance of g is 0 or ∞, whenever $ac=bd$; and this is Thomson's method of measuring the resistance of the galvanometer d.

Further, from what has been said, there is no objection to an existence of electromotive force in any or all of the conductors, *provided it remains constant*; for it will be equally possible to observe whether changes in r (of any sort) have any effect on the current in g; and if not, then $ac=bd$, as before. For the case when d is a battery of constant electromotive force, this is Mance's method of determining its resistance.

But it must now be observed that although changes in r may have no effect on the current in g, they must affect very essentially the current every where else, and therefore through the battery d. This battery ought, then, to preserve its electromotive force constant in spite of variations taking place in the strength of its current—a thing which no known battery is capable of doing. The electromotive force of every battery is really a function of the current it is producing and of the time it has lasted. In cells called *constant* the dependence of electromotive force on current and time is only slight; but in none does it disappear.

This fact that the current* and consequently, to some extent, the electromotive force of the battery are made to vary by the process of measuring its resistance, constitutes a great apparent defect of the method; but it is an irremediable defect, and is not peculiar to this particular method. It is in fact *impossible* to measure the resistance of a battery without varying the strength of the current passing through it, by any method founded, as all our methods are, on a measurement of current or of difference of potential. In other words, just as it is impossible to measure any resistance whatever without the passage of a current through the resisting body, although it is quite easy to measure an electromotive force without any current circulating through the electromotor, so, although a current of constant strength is sufficient to give a measure of the resistance of a homogeneous conductor, such as a metallic wire of uniform material, or a homogeneous liquid, or any thing else which contains no internal electromotive force, yet a variable strength of current is necessary to determine the resistance of an electromotor.

And the reason of this is apparent, viz. that the opposition experienced by a current in passing through an electromotor is of two kinds—one due to the proper *ohmic* resistance, the other due to the electromotive force; and with only one strength of current it is no more possible to tell how much of the opposition is resistance and how much is electromotive force, than it is to obtain the values of two unknown quantities from one equation. We may either take two measures of the strength of current and then eliminate one of the unknown quantities algebraically, or we may use a contrivance (like Mance's method) by which one of them (viz. electromotive force) is eliminated electrically; but two strengths of current are just as essential in the latter case as in the former, as also it is just as necessary that the two unknown quantities shall remain constant. It is possible that the resistance, as well as the electromotive force, of a battery does not accurately fulfil this condition, but that it varies to some extent with the current;

---

* Professor Clerk Maxwell, in describing this method ('Electricity and Magnetism,' i. p. 411), says that "the current in the battery is not in any way interfered with during the operation;" but this must be a mistake.

in so far as it does this, however, it is not a definite thing, and is incapable of accurate measurement.

I have entered into this matter at some length because the slip in Maxwell is getting repeated in other books (*cf.* Cumming's admirable 'Introduction to the Theory of Electricity,' p. 162); and it is as well to get clear on the subject.

The difference of potential required to force a current of strength C through an electromotor of resistance R and internal electromotive force ϵ is

$$E = RC \pm \epsilon.$$

Various methods may be applied to measure E and C; but no observation of a single value of E and C can determine R unless ϵ is known. Another observation with a different value of E and C must be made; and then ϵ can either be eliminated directly, or one can employ an indirect means of effecting its elimination, provided it remain constant. (If it does not remain constant, and if the law of its variation is unknown, no amount of experiments can eliminate it.) It is true that a single strength of current will suffice to determine R after ϵ is known; but in the determination of ϵ another and quite different strength of current (viz. zero) was employed.

A curious illustration of the impossibility of measuring the resistance of an electromotor by means of a constant current was noticed the other day in the physical laboratory at University College by Mr. H. F. Morley, who has found that the current produced by a certain form of gas-battery is, within very wide limits, almost independent of the resistance of its circuit. He endeavoured to measure the internal resistance of this battery by means of its own current, but found it quite impracticable.

Variation of the Electromotive Force of a Battery.

In what precise way the electromotive force of an ordinary cell depends on the current passing through it and on the time that current has lasted, I am not aware of any experiments which afford us information. But a law like the following seems not improbable.

$$E = Ae^{-\frac{pt}{R}} + B, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where t is the time the cell has been in action through the resistance R; so that the rate of change of E is proportional to the excess of the strength of current it is producing over the minimum strength to which it will ultimately fall, or

$$\frac{dE}{dt}=-p\frac{E-B}{R}: \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

p is a number which may be constant, or it may be a function of the current or of t; but for a cell making any pretensions to constancy, it must be small. A and B are constants such that $A+B$ is the initial and B, the final, value of E.

At any rate we may, I think, reasonably assume that E is not affected instantaneously, however much the resistance of the circuit R is changed, but that it takes a certain time to change its value appreciably; consequently, if we only change R for an instant of time and then restore it to its original value, E may be regarded as constant. It is this fact, I apprehend, which gives Mance's method its practical value, and renders it superior to the somewhat similar methods of Siemens and Thomson, because in it the change of resistance of r can be made very rapidly without disturbing the galvanometer, and need only last a few seconds. The shortest time, however, is sufficient for some variation to take place in the battery; and accordingly a kick of the needle is usually observed, like that produced by an extra current, which is very annoying. The modification which I have to propose, however, renders possible so great a virtual diminution of the period of contact that this disappears.

Modification of Mance's Method.

There is also a practical objection to the ordinary form of Mance's method, not relating to its essentials, but to its sensibility and convenience, which the modification is intended entirely to remove. It is this:—The galvanometer in g, whose function it is to indicate any change in the current in that branch, has always a certain current passing through it, and its needle is therefore deflected more or less, according to the sensibility of the galvanometer; but the current produced by an ordinary cell whose resistance one wishes to measure is usually such as one does not care to pass through a delicate instrument, even if the excessive deviation it produces be corrected by external magnets. A rough galvanometer is therefore generally employed, and the needle is brought back reasonably near its mean position by magnets placed near it. But the needle being thus constrained by immersion in a powerful magnetic field, is by no means under favourable conditions, and only comparatively large changes in the current can be indicated by it. To remedy this defect and to make the method a *null* one, my first idea was to use a differential galvanometer and to send through its second wire a current from an auxiliary battery equal and opposite to the current produced in its first wire by the cell whose resistance is being determined, so as really to neutralize instead of merely to overpower its action on the needle. Or, without using a differential galvanometer, we may balance the electromotive force in the galvanometer circuit by means of an auxiliary closed battery circuit after the manner of Poggendorff. If either of these arrangements be adopted, we can use a sensitive Thomson's galvanometer, and its needle may be as nearly astatic as we choose. But it is not easy to get the two batteries under such similar conditions that they shall constantly oppose one another exactly; and though these arrangements may be useful in some cases, they are rather complicated and the adjustments somewhat difficult to make.

The next alteration which suggested itself consisted in interposing a condenser in the galvanometer circuit (see fig. 3). This effectually prevents any continuous circulation of electricity in that branch; and the galvanometer therefore remains at zero after the condenser has acquired its full charge; but any variation in this charge is indicated by a throw of the galvanometer-needle proportional to the amount of variation. The quantity of electricity flowing into or out of the condenser through the galvanometer-coil will be equal to the variation of potential, Y, taking place between its terminals multiplied by S, its statical capacity; and the throw of the galvanometer-needle α will be proportional to this quantity multiplied by the galvanometer-constant Γ, which depends directly on the number of turns of wire on it. The resistance of the galvanometer is quite immaterial. If H is the strength of the magnetic field in which the needle hangs and T the time of a complete oscillation of the needle in that field, we have

$$\sin\frac{\alpha}{2}=\frac{\pi\Gamma SY}{HT}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

By using, therefore, a galvanometer with a very large number of turns, and a condenser of great capacity, one can increase the sensitiveness of the method to any extent.

The investigation of the distribution of currents throughout the circuit becomes very simple now that there is no continuous current through the branch g. The connexions are shown in fig. 3, where AC is the branch r, whose resistance can be changed at pleasure from infinity to something near zero. Let A, B, C, D, be the potentials of the four corners; let d be the resistance of the battery we wish to measure, ϵ its electromotive force, and u the strength of the current passing through it. We want the difference of potential B−D to be wholly independent of the potentials of A and C, which will be altered by changing r. Now as there is no current through g, we have the same current passing through b as through a—that is,

$$\frac{A-B}{a}=\frac{B-C}{b}, \text{ or } B=\frac{Ab+Ca}{a+b};$$

similarly

$$D=\frac{(A-\epsilon)c+Cd}{c+d};$$

hence

$$B-D=\frac{(A-C)(bd-ac)+\epsilon c(a+b)}{(a+b)(c+d)}, \quad . \quad . \quad (4)$$

which shows that the difference of potentials between the terminals of the condenser is independent of the potentials A and C as soon as the condition $bd-ac=0$ is satisfied.

We may conveniently write the above expression in terms of the strength of the current u passing through the battery d; thus, since $A-C=\epsilon-(c+d)u$,

$$B-D=\frac{(ac-bd)u+\epsilon b}{a+b}. \quad . \quad . \quad . \quad . \quad (4a)$$

So, if $ac=bd$, the difference of potential B−D is quite independent of the current through the cell (except in so far as the electromotive force ϵ depends upon it) and is equal to $\frac{\epsilon b}{a+b}$ or $\frac{\epsilon c}{c+d}$, which are the same thing.

The current u is of course dependent on the resistance r of the branch AC, being

$$u=\frac{(a+b+r)\epsilon}{(a+b)(c+d)+r(a+b+c+d)}; \quad . \quad . \quad . \quad (5)$$

so we may also write the above difference of potential in terms of this resistance r, thus :—

$$\mathrm{B}-\mathrm{D}=\epsilon\,.\,\frac{(a+b)c+r(b+c)}{(a+b)(c+d)+r(a+b+c+d)}. \quad . \quad . \quad (4\,b)$$

All the differential coefficients of this with respect to r contain the factor $ac-bd$; consequently when this factor vanishes this quantity is independent of r.

Conditions for Sensitiveness.

To find out what are the values of a, b, and c which give the greatest sensitiveness, we can subtract the value of B$-$D when r is infinite from its value when r is zero, and can make this quantity a maximum when the condition $ac=bd$ is nearly fulfilled. The quantity which has to be a maximum is

$$y=(\mathrm{B}-\mathrm{D})_0-(\mathrm{B}-\mathrm{D})_\infty=\frac{\epsilon(ac-bd)}{(c+d)(a+b+c+d)}. \quad . \quad . \quad (6)$$

The resistance d is supposed to be given ; so let us define the others with reference to it, putting

$$c=\lambda d,\ a=\mu d,\ \text{and}\ b\doteq\lambda\mu d=\lambda\mu(1-z)d,$$

where z is a small quantity ; then the above quantity becomes

$$y=\frac{\lambda\mu z\epsilon}{(\lambda+1)^2(\mu+1)}. \quad . \quad . \quad . \quad . \quad . \quad (6\,a)$$

Considered as a function of λ, this is a maximum when $\lambda=1$; it has in fact the same value whether $\lambda=n$ or $\frac{1}{n}$. Considered as a function of μ, it has no maximum, but it is greatest when μ is infinite, though it does not increase fast after μ is tolerably large ; the curve is, in fact, a rectangular hyperbola with asymptotes $y=1$ and $\mu=-1$; and 1 is its greatest value for positive values of μ. Accordingly the most sensitive arrangement is obtained when $\lambda=1$ and when $\mu=\infty$—that is to say, when c is equal to d (the resistance to be measured), and when a and b are equal and as large as convenient. When these arrangements are made, the maximum value of y, or the change in the difference of potential between the terminals of the condenser brought about by depressing the key, is, when c is nearly equal to d,

$$\mathrm{Y}=\tfrac{1}{4}\epsilon z \quad \text{or} =\frac{\epsilon}{4}.\frac{c-d}{d}; \quad . \quad . \quad . \quad . \quad (7)$$

and this is the quantity to be inserted in equation (3).

The sensitiveness is evidently directly proportional to the electromotive force of the cell: but it is independent of its resistance; *i. e.* a high resistance is measured with as great proportional accuracy as a low one. But it must be remembered that when the resistance to be measured is great, the resistances a and b should be as great also; if they are not as great as d, the sensitiveness falls off very appreciably. But, as said before, there is really no limit to the sensitiveness of the method; for the size of the condenser and the length of wire on the galvanometer may be increased to any extent.

Practical Details.

The condenser I have used is a small standard one with the dielectric of mica; and it has a capacity of slightly over one micro-farad. The galvanometer is a Thomson astatic by Elliott, with a resistance of about 7000 ohms. The two branches a and b of the resistance-coils, forming the equal arms of the bridge, were 1000 ohms each, being the largest resistance conveniently available in the box of resistance-coils used. But when the resistance to be measured is large (say over 500 ohms) it is better to have a and b greater than this; and they may then be made of Muirhead's carbon-paper (fig. 4). A strip 2 feet long by half an inch broad will have a resistance of about 14000 ohms; and the galvanometer terminal B may be connected with its middle so as to divide it into two halves representing a and b. Exact equality in the two arms is not essential, as it is easy (and, indeed, generally advisable) to eliminate any errors of this sort by a method analogous to double weighing. Connect a and b to a commutator in such a way that it is easy to interchange them end for end (see fig. 4), and balance the resistance d by the coils c; then interchange a and b and balance again; this time we may require an amount c'. Then it is easy to see that $d=\sqrt{(cc')}$, whatever the ratio of a to b; for in the first case we have $d:c=a:b$, and in the second $d:c'=b:a$. If c and c' are nearly equal, their arithmetic mean may be taken instead of their geometric, as being easier to calculate.

Use of a Double-wire Galvanometer as an Electrometer.

When a differential galvanometer, or a galvanometer with two long fine wires wound side by side, is employed, a separate condenser is not absolutely necessary; for the galvanometer itself has a certain capacity, and in order to charge one of its wires up to the potential B, and the other down to the potential D, a certain quantity of electricity must flow into the one wire and out of the other, and any variation in this quantity will affect the needle (though the galvanometer-constant has only half its ordinary value). Even when a separate condenser is used the capacity of the differential galvanometer may be taken advantage of, by connecting the terminals of the condenser to its two middle screws (instead of joining them directly to each other by a wire and inserting the condenser as in fig. 3), so that both condenser and galvanometer get charged instead of only the condenser. The defect of this method is, that the insulation between two silk-covered wires is not very perfect, and there is a slight leakage, which maintains a slight continuous deflection of the needle when the two outer screws are joined up to a battery; moreover the statical capacity of an ordinary fine-wire differential galvanometer is not very great.

But I think it may be often convenient to use a double-wire galvanometer as an electromoter in this way. For instance, rapidly to compare the electromotive force of any number of cells, join them up to the outer screws of the galvanometer with disconnected wires one after the other; the kick in each case measures the electromotive force of the cell. It might also be used to measure very high resistances. It is quite possible, and indeed very probable, that this method has been suggested before.

Elimination of Variations in Electromotive Force.

It has been stated above that if only momentary variations are made in the resistance r, or in the value of u, we can consider ϵ, the electromotive force of the battery, constant. The plan I adopt is to make the *effective* variation of r, or the variation which is to have any influence on the galvanometer, very short indeed. And this is done by arranging that the key m which closes the circuit of r shall break the galvanometer circuit g, the instant after, at the point n, as shown in figs. 1 and 4. For an instant, then, u is varied; and if the resistances are not balanced so that $ac=bd$, a certain quantity of electricity will enter or leave the condenser through the galvanometer; but variations in ϵ (due to the changed u), which would produce the same effect on the galvanometer, no matter how much the resistances were balanced, have no time to take place before the galvanometer circuit is broken; and then no further change has any effect. This works very well in practice; and the resistance of a cell can be really determined when producing a current through a definite resistance, viz. $a+b+c+d$. This cannot be done accurately by any other method that I know of.

Measurement of any Liquid Resistances.

The method may be applied to determine the resistance of electrolytes in general. A long tube containing the electrolyte surrounded by a jacket of water at a known temperature is interposed in the battery circuit d, the battery being one whose resistance is small and can be depended on; and the resistance of the two together, battery and tube, is measured.

The tube is then removed, and the resistance of the battery determined alone; the difference of course gives the resistance of the electrolyte in the tube. The tube can then be filled with mercury and the measurement repeated. The amount of polarization of the electrodes is of no more consequence than the electromotive force of the battery; provided the gas given off is not allowed mechanically to obstruct the current; and the effect of variations in its amount are reduced to a minimum by the method just described for the battery. It is well to make the tube end in a couple of globular receivers with two necks, very much like Dewar's electrometer, and to plunge large electrodes into these globes (see T, fig. 4). Their position in the globes is not of very much consequence; neither is a bubble or two of gas on their surface; the principal part of the resistance is offered by the liquid in the tube. If a thermometer is kept in each receiver and the liquid be allowed to flow backwards and forwards through the tube once or twice before observing, its temperature may be known without surrounding it with a jacket. But of course this does not apply accurately when it is required to raise its temperature much above that of the atmosphere. Since the sensitiveness depends on the electromotive force of the battery, it is well to use one or two Grove's cells. Fig. 4 is a complete diagram of the connexions, showing the arrangement for interchanging *a* and *b* by means of a commutator, and also showing a key which will break the galvanometer circuit at the instant required and act instead of the extemporized arrangement of two keys depicted in fig. 1. The lower spring carries a block which presses up against a screw connected with the galvanometer, except when it is forced down by the upper spring. This block is insulated from the spring, which carries it, but is connected with the point B. The interval elapsing between the breaking contact at *m* and the making at *n* is, and must be, utterly inappreciable. For the shortest practicable interval is sufficient to allow the currents to adjust themselves, unless *a* and *b* are extravagantly large resistances; and if it is not exceedingly short, disturbances will occur due to variations in the battery.

The diagrams purposely show the galvanometer, and not the condenser, connected with the breaking-key *n*, because leakage of the condenser is sure to occur to some extent when its circuit is broken, and, in order that this may not in the least signify, one terminal of the galvanometer must be insulated.

University College, London.

May 12, 1877.

XV. *On the Chromatic Aberration of the Eye in relation to the Perception of Distance. By* SILVANUS P. THOMPSON, *B.A., B.Sc.*

1. THE experimental researches of Wollaston, Arago, Fraunhofer, Young, Matthiesen, Brewster, Müller, and Helmholtz have left no doubt that the eye has the defect of chromatic aberration. Rays of light of dissimilar colour, starting in the same direction from any luminous point and entering the eye, do not all come to focus upon the retina at once, but the foci for differently coloured rays are differently situated with respect to the retina.

Hence, so far as the perception of the distance of a luminous object is dependent upon the accurate focusing of rays upon the retina of the eye, so far the want of adjustment in the eye introduces a source of error into the data of our perception. If the eye be not achromatic, a blue object and a red object of equal dimensions placed side by side cannot be in focus at once, and hence must appear either unequally distant or else of unequal magnitude.

2. The object of this paper is to demonstrate that the fact thus briefly sketched out affords a true and legitimate explanation of certain empirical rules of artistic practice relative to the pictorial expression of distance. The existence and universal recognition of such empirical rules has presented itself as a problem to the mind of the writer for the past six years; but it is only within the last few months that the solution now offered has been adopted by him.

It is proposed therefore :—

I. To enumerate the various data dependent on the eye and not upon the feet or other limbs, for the formation of a perception of distance.

II. To discuss the weight to be attached to the several data under various circumstances.

III. To inquire how far these data may be dependent upon the apparent colour of an object, or upon the formation of an exact focus upon the retina of the eye.

IV. To give a brief account of some experimental results, together with conclusions derived therefrom.

I. *Data for the Perception of Distance.*

3. The following nine data may be enumerated as contributing to the formation of a perception of distance.

4 (*a*). Apparent motion of objects caused by motion of the observer. If we move from one place to another, or alter the position of the head, the visible configuration of objects suffers a corresponding change, which by association with previous experience suggests an external system, of whose distance this "parallax" affords us an idea.

5 (*b*). Apparent change of configuration of objects according as they are viewed with one eye or the other. This affords a "parallax" similarly to (*a*), though on a more limited scale.

6 (*c*). The muscular sensation of convergence or divergence of the two eyes when directed towards an object. This is a genuine muscular perception, though guided by the optical perception of want of positional adjustment when the corresponding parts of the two images do not fall upon the corresponding points of the two retinæ.

7 (*d*). Retinal magnitude of an object as appreciated by the muscular effort of turning the eyeball so as to bring the parts of the image successively into the centre of the field of vision.

8 (*e*). Retinal magnitude as a sensation of excitement of a certain area of nerve matter of the retina.

9 (*f*). The muscular sensation of effort of the ciliary muscle in the adjustment of the eye to exact focus. The perception of exactness or inexactness of focus which guides the muscular action is, too, an optical effect in itself of little value as a means of estimating distance, except in so far as it controls the required muscular contraction.

10 (*g*). Binocular dissimilarity of the images on the two retinæ, which, conspiring to give one mental impression, present slight differences that suggest the idea of distance.

11 (*h*). Linear perspective. Under this term I propose to include, beside the usually understood play of lines and diminishing apparent magnitudes commonly spoken of as

perspective, such artificial means of estimating distance as are afforded by comparison with objects of known size (figures, cattle, &c.), and such notions of position, surface, &c. as can be drawn from comparison with houses or other objects of known form and magnitude.

12. (*i*) *Aerial Perspective*, in its widest sense. On this point Helmholtz has most aptly remarked* that the brain has the same means for the appreciation of relative distance as are employed by the painter on his canvas; and he enumerates amongst these means strong shadows, bold relief in the foreground, obscurity of atmosphere, and dimness of outline, in addition to contrasts with objects of known size or form. The term, however, is often restricted to the peculiar alteration of the colour of objects due to the intervening atmosphere †.

II. *Relative Importance of foregoing Data.*

13. It is obvious that the considerations advanced in (*a*), (*b*), and (*c*) are foreign to the present inquiry, though of enormous importance in the estimation of distances in general; for there can be no doubt that where these means are available, the mind is guided to a large extent by them in the formation of an estimate of distance, unless carefully trained to disregard them and to depend on other circumstances, or unless, as may happen to individuals, some physical injury has occurred to render the eye abnormal. With respect to the consideration advanced in (*c*), I have some reason to doubt whether we really do judge apparent distance by the muscular sensations of the external and internal recti muscles of the eye. Stereoscopic pictures appear to me equally "solid" whether in a Wheatstone's reflecting stereoscope, a Brewster's lenticular with prism-shaped lenses, or a Helmholtz's with parallel axes, or by superimposition by effort of the recti muscles, in each of which cases a different convergence is

---

* 'Popular Lectures,' English edition, p. 281.

† Mr. Ruskin's definition of aerial perspective is, from an artistic point of view, of value. "Aerial perspective is the expression of space by any means whatsoever, sharpness of edge, vividness of colour, &c. assisted by greater pitch of shadow, and requires only that objects should be detached from each other by degrees of intensity in *proportion* to their distance."—'Modern Painters,' vol. i. p. 139.

required. Moreover it would be very inconvenient if our estimate of distance depended on the action of these muscles, as they are peculiarly liable to fatigue.

14. Retinal magnitude, to use the term adopted by Wheatstone, whether regarded in its muscular (*d*) or its optical (*e*) sense, affords only an indirect means of estimating distance by association with ideas previously acquired concerning the retinal dimensions of objects of known size at known distances. Nothing is more easy than deception arising from this cause. We constantly hear the magnification of the telescope spoken of as though the enlargement of the retinal image produced the same effect as if the object were brought so near to the eye as to yield a retinal image of equal area. This is evidently not the case, as the distant object, however much magnified in the telescopic image, is still seen by rays travelling in paths nearly parallel to each other, and not containing angles as great as those contained by rays proceeding from an object really near. Hence a telescopic presentation of an object is flatter than the object itself appears when brought sufficiently near to give a retinal image of equal magnitude.

15. The fallacy of judging distances by apparent magnitude is made evident by the absurd comparisons often made between the sun or moon and other objects, some individuals comparing them to the size of a coin, a plate, or a cart-wheel. Comparatively few persons could at once tell you correctly which appears the larger, the setting sun or a threepenny-piece held at arm's length—the fact being that the angular magnitude of the latter exceeds that of the former.

16. The following experiment* illustrates a remarkable power of accommodation of the eye to the requirements of the mind in respect of retinal magnitude. Place under a stereoscope a penny and a halfpenny, having the "heads" of both coins placed upwards and similarly. You shall see but *one* coin when looking with the two eyes, especially if you expect no dissimilarity and do not use the eyes alternately. There are, on the other hand, experiments of Helmholtz, Fechner,

---

* My attention was first drawn to this curious fact by Mr. Joseph Beck, of the eminent firm of Smith and Beck.

and Volkmann* which go to prove that the retinal magnitude, as appreciated by the muscular sweep of the eye, affords a fairly accurate means of comparison of the angular magnitudes and positions of two objects in the field of vision whose images can be successively brought upon the same spot of the retina. This involves, however, the question of the persistence of visual impressions, without which such a comparison of retinal magnitudes might be quite fallacious.

17. The sensation (*f*) of tension of the ciliary muscle is a very delicate means of estimating near distances, and is a sensation capable of training to a considerable nicety. In October 1870 the writer was unable to perceive any difference of strain of adjustment to vision between distances of 4 yards and 400 yards. At the present time he is conscious of a difference of tension when the eye is directed to objects 15 yards and 300 yards distant respectively, and this with either eye. I insist on this means of estimation of distance, because it is, for monocular vision, of even more importance than the muscular sensation of convergence of the optic axes is for binocular vision.

18. The researches of Wheatstone, Brewster, Dove, Helmholtz, Brücke, Müller, Rood, Volkmann, and Wundt upon the stereoscopic presentation of objects resulting from the binocular dissimilarity of the two retinal images (*g*) illustrate the importance of this fact of binocular vision. Wheatstone's pseudoscope proved the point by a *reductio ad absurdum*; and an equally conclusive proof is obtained by cutting a stereoscopic slide in two and placing the halves in laterally reversed position under the stereoscope. I have, however, experimentally found that considerations of colour may sometimes outweigh the judgment founded by the eye upon the element of dissimilarity. I drew a pattern of two squares of four blue lines each upon paper, and within each, and outside each, centrally described circles in red. When this pattern was viewed in the stereoscope, the blue squares seemed to draw back from the eye. I then traced some of Möser's crystal forms from the well-known lithographed stereoscopic slides, colouring the *front* lines of the crystals with ultramarine, and the *back* lines

---

* Helmholtz, 'Popular Lectures,' p. 279; and *Physiologische Optik*, pp. 522–524 and 542.

with vermilion. Several, though not all, of these when placed in the stereoscope refused to appear of their true form, the blue lines, whose positions would stereographically give a front presentment, retiring behind the red lines of the crystal.

That the mind does, however, habitually make use of the dissimilarity of binocular vision is evident from the old trick of looking with one eye through a short tube while attempting to walk up to a suspended ribbon and cut it with a pair of scissors. Yet persons who have the use of but one eye find no difficulty in performing the task; and, with practice, the feat becomes easy if the distance be carefully estimated by the sensations of ocular focus. The linear distance between the two eyes, however, sets a limit to the range of habitual estimates of distance founded on the fact of binocular dissimilarity; and, as Mr. Ruskin has observed*, it is possible to see, and to see in focus together, the extreme distance and the middle distance of a landscape, though the foreground and distance cannot so be seen together.

19. I cannot here dwell upon questions involving linear perspective (*h*). The various considerations upon which linear perspective becomes a basis for the perception of distance are purely associative—mental rather than optical, geometrical rather than physical.

20. I pass on, therefore, to aerial perspective (*i*); the various definitions of which may be summed up in one as the expression of distance by colour. A distant hill looks bluer than a less distant one by reason of the slight opacity of the intervening atmosphere ("sky"); and its blueness is in some manner, to a normal eye, a far truer measure of its distance than any one of the considerations yet treated of. The blueness thus existing in nature varies with the changing conditions of atmosphere, and hence is liable to be fallacious when the atmo-

---

* Mr. Ruskin advances this point in defence of the practice of landscape-painters in leaving foregrounds sketchy and unfinished, maintaining that they should exhibit "a decisive imperfection, a firm but partial assertion of form which the eye feels indeed to be close home to it, and yet cannot rest upon, nor cling to, nor entirely understand, and from which it is driven away of necessity, to those parts of distance on which it is intended to repose."—'Modern Painters,' vol. i. pp. 183 and 184, note. Mr. Ruskin adds that Turner was the first to introduce this treatment into landscape art.

spheric conditions are unfamiliar. Thus in Smith's 'Optics' it is narrated of Bishop Berkeley that, when travelling in Italy and Sicily, he thought the cities as he approached them two or three miles too near, by reason of the unfamiliar transparency of the air.

III. *Bearing of Chromatic Aberration upon the foregoing Data.*

21. Taking the chromatic aberration of the eye as an established matter *, it is now time to inquire how far these various considerations are affected by the fact that rays of different colours have different focal length in the eye.

22. Obviously all estimates of distance which depend on retinal magnitude are liable to be influenced by colour. This may easily be verified by taking two equal disks of red and blue paper and placing them upon a black background. It will be found impossible to get a perfectly distinct image of both at once, one or other being out of focus, and therefore blurred at the edge and larger; or if no difference in size appears, one (the blue one) will appear more distant. The experiment may be varied by giving various sizes and shapes to the coloured disks, but with the same result, especially if the disks be looked at with one eye only. Orange and green disks may answer the purpose, but not so well as disks of red and blue, as these colours are most widely separated on the spectrum†.

---

* In 1835 Brewster wrote: "I consider the *non-achromatism of the eye* as a fact as well established as any other fact in natural philosophy," Phil. Mag. 1835, p. 161.

† Brewster called attention to similar phenomena produced by coloured outlines in 'Brit.-Assoc. Report,' 1848, p. 48, "On the Vision of Distance as given by Colour." He there gives a rather laboured explanation, comparing the vision of different distances for the differently coloured lines to the phenomena of the lenticular stereoscope. He adds that "the difference of distance of the coloured lines or spaces may be appreciated even with one eye." Compare also Helmholtz, *Physiologische Optik*, p. 645.

Compare also an experiment with a disk of black paper on a background of pale ultramarine, by Prof. O. N. Rood, 'Silliman's Journ.' xxxi. 1861, pp. 343, 344.

There is little doubt that Wheatstone's phenomenon of the "Fluttering Hearts" (see 'Brit.-Assoc. Rep.' 1844) is due to the attempt of the eye to focus for adjacent spaces of colours of unequal refrangibility, which

23. On this account I am disposed to think that retinal magnitude has little to do with the appreciation of distance, except in the case of objects whose magnitude and tint are familiar to us ; and I shall show other reasons for the opinion. Hence I regard as very imperfectly true the statement of the late Sir Charles Wheatstone, in the Bakerian Lecture for 1852 [*], that "Convergence of the optic axes therefore suggests fixed distance to the mind; variation of retinal magnitude suggests change of distance." I think that the muscular sensation of convergence of the eyeballs serves rather to give an idea of rate of change of distance [†] ; and I have just indicated the cases to which variation of retinal magnitude are limited.

24. In Wheatstone's classical research of 1838 it was demonstrated how great is the capacity of the brain to combine together two slightly differing retinal images. This faculty once admitted, however explained, renders it unnecessary here to discuss how far the inexactness of focus for any particular tint is concerned in binocular vision. In the present paper it must suffice to treat of the non-achromatism of the eye in its relation chiefly to monocular vision. And, for reasons stated in § 19, we shall omit further reference to linear perspective, since its relation to our perception of distance is not based upon apparent colour, or upon the formation of an exact retinal focus.

IV. *Experimental Results, and Deductions.*

25. Fraunhofer [‡] is generally credited with the first careful attempt to measure the chromatic aberration of the eye. His method of experiment consisted in placing a wire in the principal focus of a telescope, in which the focus was then obtained by the adjustment of the lenses of an achromatic eyepiece. The theoretical adjustment to distinct vision for the

---

could not, therefore, be in distinct focus at one time. The same peculiar fluttering is observed when white objects on a dark ground are observed through a purple solution of permanganate of potash.

[*] Phil. Trans. 1852, p. 4.

[†] See a curious little footnote by Mr. A. J. Ellis at p. 97 of his edition of Helmholtz's 'Sensations of Tone.'

[‡] See Gilbert's *Annalen*, lvi. 1814, p. 304; Helmholtz, *Physiol. Optik*, p. 128; Brewster, 'Edin. Phil. Journ,' xix. p. 25.

various coloured rays was then calculated and compared with the actual adjustment ; and the difference between theory and observation was assumed to measure the aberration of the eye. The method is at best only indirect ; and there appears to be some doubt whether in the critical experiments an achromatic object-glass as well as an achromatic eyepiece was employed. Nevertheless, under the conditions of the experiment, the measurements were very accurately made, and are therefore invaluable.

26. I have found the most satisfactory exhibition of chromatic aberration to be manifested when a solution of permanganate of potash was employed, in a narrow glass trough, as a medium to intercept the middle rays of the spectrum. I have also tried solutions of iodine, and cobalt glasses of various depths of tint ; but none are so satisfactory. For some experiments I have employed a piece of thin cobalt glass in conjunction with a solution of permanganate. By this means I have obtained media transmitting red and blue-violet rays freely, but totally opaque to all rays between D and E, and nearly opaque to rays from C to D in the orange, and from E to F in the green and blue.

27. A silvered bead placed in the sunlight, and viewed through such a medium can by no possibility be seen with accurate focus for all rays. It appears either as a near red point of light surrounded by a blue haze, or as a distant blue light surrounded by a red haze *. The point of light seems to change distance from far to near, or from near to far, with the effort of the eye. This effect, however, is only to be well observed when the intensities of the red and blue rays are about equal ; for if either predominate, the eye will focus for the brighter unless special care is taken to adjust and keep the required focus.

28. The researches of Jurin, Schreiner, Müller, Powell, and Trouessart have tended in favour of the view that the eye is achromatic at least for objects at the centre of the field of vision, when distinctly in focus. The fundamental illustration of this view is as follows :—Take any visible white object—say

---

* This effect may be shown objectively to an entire audience by casting beams of light from a lamp through such a purple medium upon a silvered ball.

a square inch of white paper upon a black background. If the eye be focused for a distance beyond the white surface, it will appear blurred at the edges, being tinged outwardly with orange-yellow, inwardly with blue. But if the eye be focused for a point nearer than the white surface, it will appear tinged outwardly with blue, inwardly with orange-yellow, while at exact focus these bordering tints disappear. To explain this seeming achromatism of the eye it was argued by Wollaston, Young, Müller, and Matthiesen that the rays passed almost without refraction along the axis of the eye, and therefore suffered no dispersion *. On the other hand, in my own experiments with permanganate-of-potash solution to intercept the yellow and green rays, I have never been able to obtain a luminous surface small enough to be free from aberration at the edges—which proves that the focus is really not exact. The rays of the middle of the spectrum, being more intense, mask the effects of the feebler rays; and the eye focuses for the more intense light, as I have mentioned in § 27.

29. If, on the contrary, a weak solution of picrate of potash be employed as a screen to cut off the extreme red and violet rays, it is very difficult with *any focus* to observe the tinted margins.

30. Moreover the apparent achromatism of the experiment above mentioned is only found with pencils nearly coincident with the axis of the eye. Oblique pencils always give tinted margins in white light. This may be verified by looking at a set of concentric circles in white and black. If the eye be steadily focused for the white centre, no colours are seen at its margin, but may be observed at the inner and outer edges of the other circles. I have always found it possible to get yellow exterior margins to a white object at any distance; that is, I have always found it possible to focus for a further distance; but not *vice versâ*.

31. If a square of white paper be carefully tinted around the edge with blue, and laid on a black ground, it appears

---

* Baden Powell contended that the refraction and dispersion of the vitreous humour being in the opposite sense to that of the crystalline lens, aqueous humour, and cornea, compensated the dispersion for axial pencils.

further off than a perfectly white square of the same size beside it, since the blue margin helps to correct the yellow rays that surround the image at a longer focus. In short, a blue-edged square appears like a white square looked at with too near a focus. The opposite effect is observed with a margin tinted yellow.

32. I adjusted a lenticular stereoscope to easy focus, then placed in it a sheet of white paper having two equal circles 1 centim. in diameter, described upon it at the appropriate distance—one red, the other blue. The joint effect of these two gave, apparently, a combination of a red circle with a slightly larger one of blue. On drawing a slightly smaller circle of blue, with the circle of red as before, they appeared to give exact coincidence.

33. Knowing the power of the eye to reconcile retinal images in magnitude (see § 16), I placed in the stereoscope a penny coloured blue and a halfpenny coloured red. They refused to be seen as one object. On replacing them by a red penny and a blue halfpenny, coincidence was, with some difficulty, obtained.

34. My next experiments attempted the direct estimation of the distance of objects by the focus of the eye apart from other means of estimation. A conical tube of metal, blackened interiorly, was taken, through which only a limited field of view was possible, the eye being placed at the smaller aperture. In front of this tube a screen of blackened cardboard was placed, capable of adjustment to any distance from 10 centims. to 350 centims. from the tube, and covered the entire field of view. Coloured glasses could be introduced in front of the tube. The objects displayed upon the cardboard screen in the centre of the field of vision were irregularly shaped pieces of white paper. These were cut by an assistant and adjusted—their distance and size being unknown to the observer, and varied between each experiment. Hence, under the conditions given, the eye could form no judgment of distance based upon apparent magnitude, upon parallax, or upon apparent form or shadow ; the estimation depended solely upon the muscular sensation of adjustment to focus. After a little practice this became moderately easy with white light, and the estimations were fairly within limits. With a screen

of yellow glass interposed no difference was experienced. When a ruby-red glass was employed, however, the estimations were generally, though not invariably, too small, and with a cobalt-blue glass generally (though not invariably) too great. The following is the mean of six experiments taken at random from a number made with a red glass, and of another six from those made with a blue glass.

	Red.	Blue.
Mean real distance	208 centims.	178 centims.
Mean estimated distance	163·1 „	199 „

35. When a solution of permanganate was employed in conjunction with a blue glass, giving equal intensities of red and blue rays, the attempt to estimate the distance of the white pieces of paper became perplexing in the extreme. The object seemed to be moving backwards and forwards as the focus changed from red to blue and blue to red.

36. After about forty experiments with blue and red glasses, the errors became less. Evidently the effect of practice was to sophisticate the result, the mind knowing the probable result beforehand, and making allowance involuntarily. The forty experiments were not all made at one time, because the eye becomes fatigued when experimentation is prolonged ; and as in my own case with fatigue partial astigmatism sets in, I was careful to test my vision from time to time, to avoid error from this source *.

37. Every book on Art will tell us that blue is a "retiring" colour, while red is an "advancing colour." For long I had been convinced that these alleged characteristics were something more than associations with blue sky and red rocks ; and the universality and tenacity with which artists pronounce

---

* Since the experimental results recounted in this paper have been chiefly obtained with my own eyes, it may be well to state the following particulars :—For objects in centre of field of vision there is no trace of colour-blindness ; neither eye is astigmatic when not fatigued ; their range of accommodation is from 11 centims. to infinity, and with good definition at all ranges ; the blind-spots are distinct, the foveæ centrales well formed and sensitive ; there is no difficulty in perceiving Purkinje's figures and Haidinger's brushes and lavender streaks. Let me add, as a test of definition, that on a clear night I can easily see twelve stars of the Pleiades.

this opinion is surely indicative of a reality somewhere. The opinion of Goethe on this point sums up the whole matter. I quote from Sir Chas. Eastlake's edition of the 'Farbenlehre'[*].

"As the upper sky and distant mountains appear blue, so a blue surface seems to retire from us."

"Rooms which are hung with pure blue appear in some degree larger, but at the same time empty and cold."

38. So the blueness of the lower misty air may, as Alpine travellers know, cause the base of a mountain actually to appear *more distant* than its summit. Mr. Ruskin has noted this in his 'Modern Painters'[†]; and the fact has been familiar to artists for centuries, having been described by Leonardo Da Vinci in his *Trattato della Pittura*, and is alluded to by Goethe.

39. The sun and moon when red at setting or rising, and the moon when red in total eclipse, look nearer—or, some people say, larger—than at other times.

40. For the sake of giving prominence or nearness to the foreground of a picture, painters will contrive to introduce a scarlet flower, or a patch of red rock, or a figure dressed in red—a practice sanctioned and well known in Art, but quite inexplicable except on the train of reasoning we have been attempting to follow.

41. The immense importance of these facts of physical and physiological optics has long been recognized in Art. The study of aerial perspective has been carried long since to a pitch that renders it worthy to be put upon a basis more than merely empirical. There can be little doubt that other empirical laws may be similarly accounted for. Visitors to picture-galleries may be constantly observed scanning a landscape through a rolled up catalogue, or under the arched fingers of the hand. Is there not an explanation, too, for this? The square frame of gold asserts the flatness of the surface, and prevents the mind from realizing that which the eye perceives—that all the rays of light do not focus at once upon the retina, but that each tint retires or advances to its

---

[*] Goethe's 'Theory of Colours,' translated by Sir C. L. Eastlake, R.A., F.R.S. (London: 1840), p. 311, par. 780 and 783.

[†] Vol. i. p. 182.

own appropriate distance. Landscapes without figures or architecture, especially, are improved by this method of treatment; and the reason is plain: in them the eye judges the scene by colour, not by retinal magnitudes or binocularly compound sensations.

42. I have endeavoured to apply the method of experimentation in another direction. Take any picture in which there are well-expressed foregrounds and distances, and look at it through the coloured media employed in the earlier experiments. If the facts of distance are expressed only indirectly, by suggestions of magnitude, by linear perspective of vanishing lines and the like, the interposition of a red or a blue glass will produce little effect, except that the latter may deaden the intensity of the shadows. If, however, the expression of distance in the picture is accomplished chiefly by aerial perspective (that is to say, by colour), a red glass will almost destroy the intelligibility of the picture, while a blue glass will draw out the distances in a marked manner.

43. Reflecting how useful is the purpose subserved thus by the non-achromatism of the eye, I consider it probable that if the eye were so constructed as to be originally achromatic, having usually blue distances and red-brown foregrounds to look at, it would, by an inevitable process of natural selection, develop into a non-achromatic instrument.

Summary and Recapitulation.

44. I would conclude therefore :—

(α) That the muscular sensation of the adjustment to focus of the lenses of the eye affords a possible means of estimating distances.

(β) That when binocular methods, and those depending on association of visible form or magnitude fail, the eye falls back on colour as a means of estimating distance.

(γ) That estimates of distance founded on apparent magnitude are liable to be rendered fallacious by the colour of the object.

(δ) That, conversely, estimates of distance founded on colour are liable to be confused by apparent magnitude.

(ε) That colour may in some cases outweigh, as a criterion, the evidence of binocular vision.

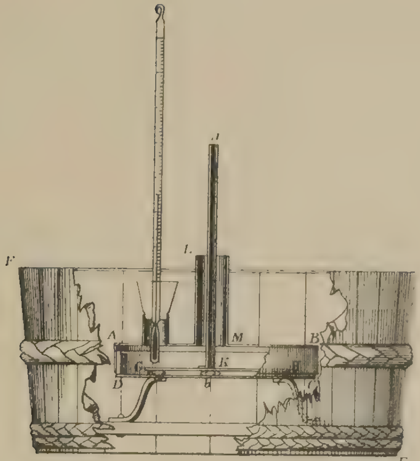


Fig. 1
Ice as an Electrolyte

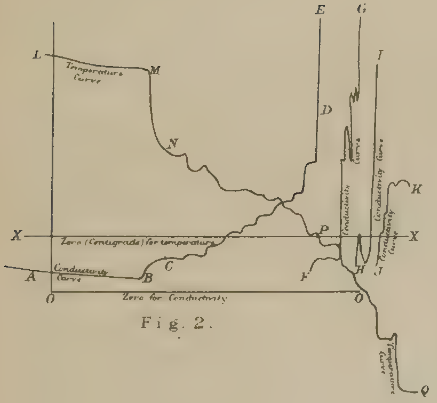


Fig. 2.

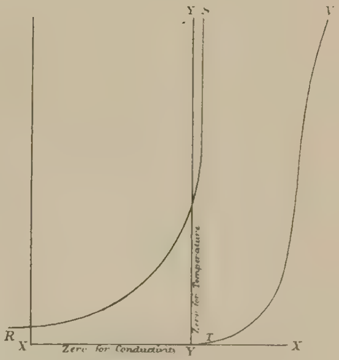


Fig. 3.

(ζ) That the chromatic aberration of the eye accounts for the universal opinion of painters as to the "retiring" character of blue, and the "advancing" character of red tints.

(η) That Aerial Perspective in Art is a true expression of a physical fact in the perception of distance.

University College, Bristol,
May 8, 1877.

XVI. *Ice as an Electrolyte. By* W. E. AYRTON *and* JOHN PERRY, *Professors in the Imperial College of Engineering, Tokio, Japan.*

[Plate III.]

FOR the purpose of measuring the resistance of ice at various temperatures, its power to act as an electrolyte, and its specific inductive capacity, the following piece of apparatus was constructed. A B C D (Plate III. fig. 1) is a copper box 17·4 centims. in diameter, rigidly fixed by means of three legs in the inside of a wooden tub F F. Resting on the bottom of the copper box, but separated from it by three small pieces of glass (*a*) (*b*) (*c*), is a copper disk, G H, 13 centims. in diameter, to which is fastened a copper strip J K. The box is closed by a very tightly fitting cover furnished with two openings—one, L M, to allow J K to pass through without touching the cover, the other for the admission of a thermometer. Distilled water having been introduced into the copper box to a sufficient depth to cover the disk G H, the temperature could be lowered by the introduction of a freezing-mixture of snow and salt inside the tub above and below the copper box, great care being taken that none of the mixture fell into the box through the tube L M, which was necessarily left open.

I. *Preliminary experiments.*—The current from one Meidinger cell was passed through the ice and a reflecting-galvanometer, the space between the copper plates being 2 millims. Keeping the temperature nearly constant (it varying between $-18^\circ{\cdot}2$ and $-17^\circ{\cdot}2$ C.), we found that in one hour the galvanometer-readings increased from 46·8 to 168·8, corresponding to a diminution in resistance per cubic centimetre of the ice from

354 megohms to 98 megohms. The cause of this may have been that, with such thin glass separating the copper disk from the box, some tilting may have occurred at freezing, so that the coppers were not perfectly parallel; or it may have been due to a very little of the salt water of the freezing-mixture having found its way into the copper box by passing between the box and the cover. The box was therefore opened and cleaned, fresh distilled water put in, and the cover cemented to the box by "cap-cement" to avoid the possibility of the salt water entering the box.

II. The current from eighty-seven porous Daniell's cells joined in series was passed through the ice, the temperature being kept very nearly constant at $-8^\circ{\cdot}0$ C. The galvanometer-reading fell regularly from 28·5 to 12·73 in twenty-seven minutes, being equivalent to an apparent rise in the resistance per cubic centimetre of the ice from 3767 megohms to 8443 megohms. The temperature remaining constant, the reading fell to 7·20 in about two hours, corresponding with a specific resistance of 17,310 megohms. Disconnecting the battery and joining the coppers on the two sides of the ice through the galvanometer, a discharge-current, which rapidly diminished, was obtained, as was to be expected.

Further experiments showing the difficulty of maintaining the copper disk approximately parallel to the bottom of the box, the three pieces of glass were removed and thicker pieces, 0·324 centim. thick, put in their place. The cover was again cemented to the box, this being, in fact, always carefully done after each occasion on which it was necessary to open the box.

III. Temperature being $2^\circ{\cdot}4$ C., an electromotive force of 0·0087 volt gave (after waiting eleven minutes to allow the current to overcome any previous polarization) a deflection of 212, corresponding with a resistance of 355,000 ohms per cubic centimetre. Allowing the temperature to fall slowly, we found the readings to remain pretty steady for thirty-four minutes when the temperature had become $-0^\circ{\cdot}6$ C. From this time there was a moderately rapid increase of resistance up to 14 megohms per cubic centimetre after forty-three minutes, the temperature now being $-3^\circ{\cdot}8$ C. The deflection was now 54; and we increased the battery ten times, when it was found that this increase of the battery did not increase the deflection proportionately (although subsequent experiments showed that the deflection was very approximately proportional to the current); in fact, making the battery ten times as large (that is, now using an electromotive force of 0·087 volt), only gave a deflection of 129. The battery was now immediately reduced to an electromotive force of 0·0087 as before; but the deflection was now only 28. Leaving on this electromotive force, the deflection was observed to steadily diminish until at last it became negative; and it eventually required an electromotive force of 0·0435 volt to bring the spot of light to zero. This electromotive force in the ice, which appeared to be developed by the employment of an electromotive force of 0·087 volt for only about one minute, remained quite constant, and continued to balance an electromotive force of 0·0435 volt for about half an hour, when, the apparatus having been cooled down, a larger electromotive force was now employed.

An electromotive force of	gave a deflection of	at a temperature of
0·087 volt	9·5	−5°·1 C.
0·174 „	19·5	
0·261 „	27·7 diminishing to 23·5 in thirteen minutes	−6°·4 C.

These deflections are nearly proportional to the electromotive forces.

An electromotive force of 2·61 volts	gave a deflection of 52·5 diminishing to 30·1	at a temperature of −8°·3 C.
Now suddenly increasing this to 4·35 volts	the deflection diminished to 39·5	−8°·3 C.

It might have been expected that doubling the electromotive force would have more than doubled the deflection, since the diminution of resistance in a dielectric produced by time of charging only proceeds gradually—whereas the deflection only increased from 30·1 to 39·5. This peculiarity is shown more fully in the following Table, in which the readings just before and after changing the battery-power are given, the intermediate ones being omitted:—

Electromotive force 2·61 volts.

Time.	Deflection.	Temperature.
h m s		
7 48 30	30·1	−12° C.

Electromotive force of battery increased to 4·25 volts.

7 49 30	39·5	−12° C.
7 51 30	36·5	

Electromotive force increased to 8·7 volts. First swing off the scale, but very soon grew steady on returning.

7 52 0	53·1	−12°·2 C.
7 53 0	50·5	
7 54 0	49·1	

Electromotive force increased to 17·4 volts. First swing off the scale.

7 55 0	72·7	−12°·5 C.
7 56 0	69·0	
7 61 0	59·5	

Electromotive force increased to 28·71 volts. First swing off the scale.

8 2 0	76·5	−12°·8 C.
8 5 0	68·6	
8 6 0	67·2	−13°·0 C.

Short-circuited the coppers on the two sides of the ice for four minutes.

Afterwards electromotive force increased to 87 volts. First a great swing off the scale, then returned, and in about one minute—that is, at

h m s	Deflection.	Temperature.
8 12 0	212·7	−12°·85 C.
8 15 0	145·7	
9 12 0	66·2	−10°·8 C.
9 17 0	65·2	
9 58 30	66·2	− 8°·85 C.

Comparing the observation at $8^h\ 12^m\ 0^s$ with that taken at $8^h\ 6^m\ 0^s$, we see that the deflections are nearly proportional to the electromotive forces employed; this, however, is not the case in the other instances when the electromotive force was increased. The object of giving three or four time-observations for each electromotive force in the above Table is for the purpose of showing with what rapidity the reading was changing in each case.

IV. *February* 11.—A very consistent series of observations was made. We first tried the effect of varying the time of charging when measuring the discharge from the arrangement as a condenser; and we found that increasing the time of charging increased the discharge, the temperature of the distilled water in the copper box being 8°·7 C. Charging, however, for ten seconds with an electromotive force of 0·174 volt, and discharging through the galvanometer when shunted with the one-thousandth shunt, and leaving the copper disk and box short-circuited for fifteen seconds after each discharge, we obtained, during many successive trials, swings varying between 75 and 81, and having a mean value of 79. This corresponded with a capacity for the water of 1881 microfarads, or 4·384 microfarads per cubic centimetre, making the specific inductive capacity of water 50×10^6, that of air being called unity. Of course this number is rather too low, as no allowance is made for the loss of charge that must occur in the very short interval between the conclusion of the charging and the commencement of the discharge. The current produced by an electromotive force of 0·174 volt was now passed through the water, and the fall of current measured as the temperature was lowered. This diminution of current was partly due to polarization, but more due to increase of resistance by diminution of temperature. It was quite evident that this increase of resistance was very great, and that it was quite regular in the passage from the liquid to the solid state. These observations we do not give; it is sufficient to state that they were consistent with the more important subsequent observations given below. When the temperature had fallen to −13°·5 C., we stopped the resistance-tests to take the capacity of the ice. We charged for ten seconds with an electromotive force of 0·174 volt, and short-circuiting the ice for fifteen seconds after each discharge, exactly as was done with the water. We now found, however, that when using the one-thousandth shunt there was no visible motion of the spot of light on discharging; and when no shunt was employed the swings only reached 25. Now, disregarding the error produced by varying the shunt in capacity-testing (a correction for which could be made in the way explained by Mr. Latimer Clark, Journ. Soc. of Teleg. Engineers, vol. ii. 1873, p. 16), and also neglecting the frictional resistance of the air, it follows that the capacity of ice at $-13^\circ{\cdot}5$ C. is to the capacity of water at $+8^\circ{\cdot}7$ C. as 25 to 79,000, or as unity to 3160. This result, however, has to be corrected for the effects of an opposing electromotive force due to some slight oxidation of the copper on the sides of the ice, and which equalled 0·053 volt. Applying this correction, we find the ratio of the capacities to be as unity is to 2240. This makes the capacity per cubic centimetre of ice at $-13^\circ{\cdot}5$ C. to be 0·002 microfarad, and the specific inductive capacity 22,160, that of air being called unity.

Substance.	Approximate capacity per cubic centimetre in microfarads.	Specific Inductive capacity approximately.
Air	8831×10^{-11}	1
Ice at $-13^\circ{\cdot}5$ C.	196×10^{-5}	22160
Distilled water at $+8^\circ{\cdot}7$ C.	4·384	50×10^6

This furnishes an additional example of the principle recently pointed out by us, that *low* specific resistance is associated with *high* specific inductive capacity.

For further experiments on the capacity of the ice at different temperatures &c., see further on.

At 12 hours 30½ minutes the current produced by 2·61 volts was passed between the copper plates, the ice being at a temperature of $-13^\circ{\cdot}6$ C. The temperature was allowed to rise very gradually; and time-readings of the galvanometer deflection were taken. From these the curves A B C D E, F G, H I, J K (fig. 2) have been constructed, horizontal distances representing time on such a scale that from the point A to the point E the time was 207½ minutes, and vertical distances measured from the line O O to the curve A B C D E representing conductivity, the specific resistance per cubic centimetre at B, for instance, being 2240 megohms. F G is a continuation from E one tenth as great for vertical distances (that is, for conductivity), but on the same scale for horizontal distances. Again H I is a continuation from G on a scale one hundredth as great for vertical distances as in the curve A B C D E, and J K on a scale for vertical distances one thousandth as great, the scales for horizontal distances in all the curves being the same. The curve L M N P Q is drawn so that vertical distances measured from the line X X represent temperature, and horizontal distances time—the zero for time, and the length corresponding with one minute, being the same as for all the other curves; negative temperature is represented upwards and positive temperature downwards—the point L corresponding with a temperature of −13°·1 C.

The current continued to diminish regularly to the point B, when the temperature shown by the point M was −12°·3 C., and the time 10 minutes past 3. At this point and at the other steep places in the curves, water was thrown into the freezing-mixture to raise its temperature; and at such points as C, where the current had evidently reached a maximum at a corresponding temperature N, the readings are supposed to represent the true conductivities at the corresponding temperatures; but this, of course, is only approximately accurate. It is very striking how the maxima of conductivity and temperature correspond with one another, considering that the thermometer only indicated the temperature of the air in the copper box above the ice, whereas change of current indicates change of state in the ice itself. The very great increase in conductivity which occurs at the melting-point is seen to be quite regular, and unlike what one might expect from the discontinuity of the solid and liquid states.

Taking the corresponding conductivities and temperatures at points C, N, &c., we have drawn the curves R S, T V (fig. 3). Horizontal distances to the right of Y Y represent positive temperatures, and to the left negative, on such a scale that the point R corresponds with a temperature of −12°·4 C. Vertical distances measured from X X represent conductivity on such a scale that the point R corresponds with a resistance per cubic centimetre of 2240 megohms. T V is a continuation from S on a scale for vertical distances three thousandths of that employed in the curve R S, the scale for temperature remaining as before: the point V thus corresponds with a resistance per cubic centimetre of 0·33 megohm at a temperature 11°·02 C. From the curves (fig. 3) it would appear that the very great increase in conductivity occurred at about 1° C.; it is possible, however, that the apparently corresponding points of maximum temperature and maximum conductivity shown in the curves (fig. 2) may not have really exactly corresponded, although changes in the temperature always accompanied changes in the current.

The curves R S, T V do not appear to be logarithmic. The coordinates of the points from which they have been drawn are given in the following Table:—

Temperature, in degrees Centigrade.	Resistance per cubic centimetre, in megohms.
−12°·4	2240
− 6·2	1023
− 5·02	948·6
− 3·5	642·8
− 3·0	569·3
− 2·46	484·4
− 1·5	387·6
− 0·2	284·0
+ 0·75	118·8
about + 2·2	24·8
+ 4·0	9·1
+ 7·75	0·54
+11·02	0·34

V. The copper disk G H (fig. 1) was now removed and replaced by a disk of zinc of exactly the same size. The pieces of glass used to separate the zinc disk from the bottom of the copper box were the same as those previously employed, being 0·324 centim. thick. Distilled water was poured in so as to cover the zinc plate, and the cover was cemented on the box as before. In the following experiments, K J (fig. 1) was a strip of zinc cut out of the same sheet as the disk G H and bent up: the strip K J and the disk G H were therefore continuous without joint. The point J was joined to one of the electrodes of a Thomson's quadrant electrometer, and the copper box D C to the other electrode. Both with water and with ice the maximum electromotive force obtained was one volt about.

In the first sets of experiments, A, B, C, the zinc disk and copper box were alternately joined together and insulated from one another, the result of the short-circuiting being, of course, to diminish the electromotive force by polarization. While this was being done the temperature was gradually lowered. It is rather difficult to determine how much the results were affected by polarization and how much by the lowering of the temperature. We give, however, the results (in a tabulated form) as we obtained them:—

A.

Time of short-circuiting, in minutes.	Deflection immediately on insulating.	Highest deflection.	Time taken to arrive at highest deflection, in minutes.	Temperature, in degrees Centigrade.
¼	.. ...	240	1st time quickly 2nd time slowly	about 0°
¼	......	240	3	−4°
¼	......	207	2	falling gradually.
¼	.....	203	......	
¼	......	196	......	
¼	... ..	190	......	
¼	.. ...	178	......	
3	......	176 diminishing to 89	quickly.	−12°
		B.		
1	nearly 0	210	8	15° to 1°·5
1	”	151	9	−1° to −5°·5
1	”	170	...	−6°
		C.		
1	184	203	5½	+2°·6
1	184	203	5½	+2°·6
3	168	204	...	+2°·6
7	156	204	...	+2°·6
10	150	167	1	−1°·5 to −2°·7
1	153	175	6	−3° to −3°·5
3	154	179	7	
7	......	195·5	25	−5° to −9°·5
7	......	208·5	60	−12°

It is possible that the deflection 208·5 would always have been obtained had the zinc plate been left sufficiently long insulated after short-circuiting. The plate and box, however, were always short-circuited when the rise in the electromotive force had apparently ceased.

To separate, to a certain extent, the effects due to polarization from those due to difference of temperature the following experiments were made. The zinc plate and copper box were alternately short-circuited for two minutes and insulated for two minutes, the temperature being kept constant at +22°·5 C. This being repeated six times, reduced the deflection 231, obtained at the end of the first two minutes of insulation, to 155, obtained at the end of the last two minutes of insulation. Exactly the same experiment being repeated ten times with ice at a constant temperature of about −17° C., the deflection 212, obtained at the end of the first two minutes of insulation, was only reduced to 203 at the end of the last two minutes of insulation. It would therefore appear from this set of experiments, that the smaller amount of polarization produced by the high resistance of the ice only allowing a small current to flow during the short-circuiting of the zinc plate and copper box was of rather more importance than the solidity of the ice retarding dissipation of polarization.

VI. *Further experiments on the Specific Inductive Capacity of Ice.*—The zinc disk G H (fig. 1) was now removed and the original copper disk replaced. With a charging electromotive force as small as 0·174 volt, we found that, in the case of ice, it was always necessary to carefully correct the swing for the opposing permanent electromotive force existing between the copper disk and the copper box, whereas with water at about +10° C., when using the same charging electromotive force, it was not necessary to make any such correction. Rough measurements gave this opposing electromotive force at −12°·4 C. as 0·017 volt, and showed that as the temperature rose there seemed to be a regular decrease: at 0° C. it was only 0·003 volt, and at higher temperature it was immeasurably small by the rough method we employed. To avoid this correction being of so much importance, we used an electromotive force of 0·87 volt to determine the capacity of ice at different temperatures. We did not find that the time of charging or the time of short-circuiting affected our results nearly as much as in the case of water; in fact, the time of short-circuiting our ice condenser between successive tests of its capacity hardly affected the results at all; but during a series of observations, the time of short-circuiting between every two being fifteen seconds, and the time of charging being gradually increased from five seconds up to thirty, the swings increased from 56 to 70 divisions of the scale. We therefore, as on the former occasion, always charged for ten seconds and short-circuited for fifteen. Four or five consecutive observations of this kind always showed a slight increase from the first to the last, the temperature being kept quite constant. Several series at different temperatures are shown in the following Table:—

Time.	Temperature, in degrees Centigrade.	Swings, first and last of a series of four.
h m 9 20	−9°·5	61 61·5
9 25	−8·8	57 61
9 30	−7·7	58 60·5
9 37	−3·5	57 62·5
9 44	−2·5	60 61
9 55	0	47·5 52·5

In the above experiments the ice condenser always remained short-circuited while the temperature was being raised between each set of experiments. The results obtained appear to show that the specific inductive capacity of ice does not materially alter from −9°·5 C. to −2°·5 C.; the apparent change near the melting-point is very probably due to the increased conductivity of the ice allowing much of the charge to be lost before the discharge through the galvanometer. Our preceding experiments have shown that the conductivity of $\left\{\begin{matrix}\text{ice}\\ \text{water}\end{matrix}\right\}$ increases regularly *without discontinuity* from −10° C. to +10° C., although altering very rapidly at 0° C., where the change of state occurs. The specific inductive capacity, on the other hand, appears to change very little while the dielectric (ice) is solid, and perhaps also changes very little while the dielectric (water) is liquid. It must therefore probably undergo a great change at the melting-point, since we have shown that the specific inductive capacity of water at 8°·7 C. is about 2240 times that of ice at −13°·5 C. The specific inductive capacity seems therefore to be a stress and strain phenomenon, and to be intimately connected with the rigidity of the body; whereas the connexion between conductivity and rigidity seems to be less marked. This, we think, bears out all recent theories in molecular physics. The complete investigation of the connexion between the conductivity and specific inductive capacity of $\left\{\begin{matrix}\text{water}\\ \text{ice}\end{matrix}\right\}$ at different temperatures will form the subject of a second paper.

March 22, 1877.

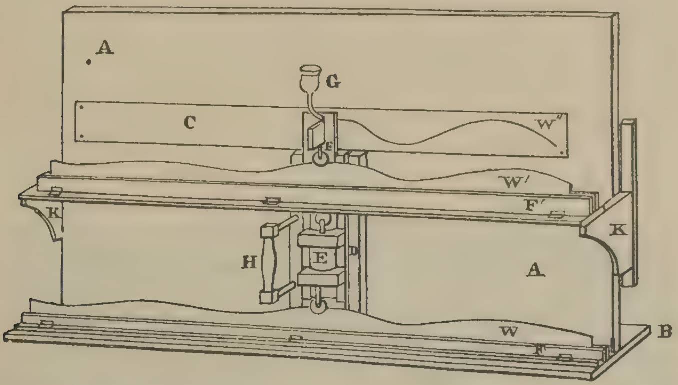
XVII. *On an Apparatus to illustrate the Interference of two Plane Waves.* *By* C. J. Woodward, *B.Sc.*

The effect produced by the simultaneous propagation of two or more plane waves is in a few simple cases easily realized; in others, however, the mind is somewhat harassed in ascertaining what will be the resultant wave made up of certain given elements. The apparatus I am about to describe is intended to assist the student by enabling him to draw for himself the required resultant wave; or the apparatus may be used in the class-room to illustrate the general subject of interference.

So far as I know, the only apparatus of the kind in use by lecturers is one based on the method described by Dr. Young (Natural Philosophy, Lecture xxxiii.); but here, to obtain any variety of effects, a series of dissected waves are required, and these are troublesome to make or expensive to purchase; whereas with the arrangement I now exhibit to the Society, when once the framework of the apparatus is made, combinations of any desired waves may be drawn, with merely the trouble incidental to cutting out the required waves in cardboard or thin tin.

The apparatus consists of a board, A A, about 3 feet long, 2 feet wide, and 1 inch thick, set upright on a base-board B. A strip of cardboard, C, is fastened by drawing-pins to the board; and on this is drawn the compound wave resulting from the two component waves W, W′. These component waves are cut out of strong cardboard or tin, and can be easily and quickly placed in position by letting down the flaps F, F′. D is a stout mahogany board, with a wedge-shaped groove in it sufficiently wide to allow the board E to slide freely up and down. A handle, H, is secured to the board D to enable the operator to move D from one end of A to the other. The board E has a pulley fastened to its lower part; and this pulley runs on the tin wave W, so that if the board D be moved horizontally, the sliding board E will rise and fall as the pulley passes over W; and of course, were there a pencil attached directly to E, the pencil would trace a curve the same, or very nearly the same, as that of the wave. The sliding board E has also a wedge-shaped groove cut in it to allow the board F to slide vertically; and to F is also attached a pulley, which runs on the wave W′. With the upper board F, as with the lower, if the pulley be made to traverse the wave W′, a pencil fastened to F would trace a wave corresponding to W′. The pencil (or, more properly, pen) used in the apparatus consists of a thistle-funnel, with the tube drawn out so as to have an

Fig. 1.

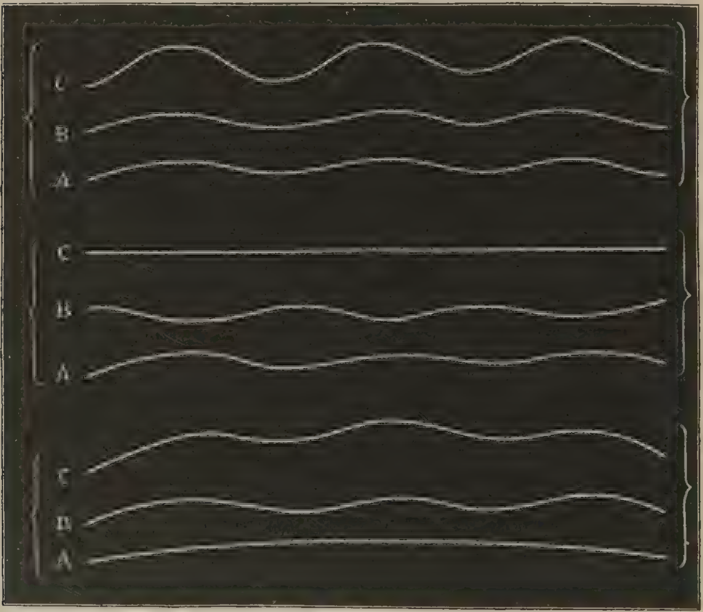


Moving the boards D, E, F from right to left by means of the handle H traces the wave W″, a combination of the waves W and W′.

opening of only a fraction of a millimetre. The tube is bent as shown in the figure at G; and the drawn-out part presses gently against the cardboard C. To charge the pen, some violet ink is poured into the thistle-funnel, which is then closed with a cork.

The action of the apparatus will easily be understood. As the board D moves along, the board E rises and falls; and this by means of a roller causes the wave W′ to rise and fall to each elevation and depression of the wave W below. Now the board D as it moves along carries with it the sliding board F with the attached pencil; and as this board F is carried along it rises and falls to the elevations and depressions of the wave W′. But, as has been said, it also rises and falls to the wave W: the motion of the pencil is therefore the algebraic sum of the forms of waves W and W′. In the figure the waves W and W′ are of the same length and amplitude and correspond in phase, so that the tracing W″ is a wave of the same length but twice the amplitude of the waves below.

Fig. 2.



The wave C results in each case from the superposition of the waves A and B immediately below C.

By moving the wave W′ to the right, its phase may be made $\frac{1}{4}$, $\frac{1}{2}$, &c. of an undulation different, and a fresh tracing obtained. By removing the wave W′ and inserting successively others of different lengths and amplitudes, various combined waves are traced out on moving the board D along. A few tracings obtained by the apparatus are represented in fig. 2, page 184.

In order that the frame carrying the wave W′ may rise and fall freely, the brackets K, K are linked together at the back of the board by a parallel motion, used in the Cowper printing-machine, and which will be found described in Goodeve's 'Elements of Mechanism,' 3rd edit. page 66.

It is obvious that by an extension of the same principle an apparatus could be made to give the tracing of a combination of three or more waves. To do this, however, the mechanical arrangements must be much better than in the comparatively rough instrument I have exhibited.

XVIII. *On Interference Fringes within the Nicol Prism.* *By* Silvanus P. Thompson, *B.Sc., B.A.*[*]

1. In the 'Edinburgh New Philosophical Journal' for 1828 (p. 23) is a communication "On a Method of so far increasing the divergency of the two Rays in Calcareous Spar that only one Image may be seen at a time." In this, the original account by the inventor of the now well-known Nicol prism, occurs the following sentence:—"There is a tinge of blue where the ordinary ray vanishes on one side, and a tinge of orange accompanied by a number of obscurely coloured fringes, where it terminates on the other." Probably the great majority of those who are familiar with the Nicol prism are acquainted with the phenomena alluded to in the passage quoted. As the author has not found, however, any more explicit reference to them, nor any attempted explanation of them, he ventures to offer the following account of an examination he has made of their nature and origin.

[*] Read June 9, 1877.

General Description of the Phenomena.

2. Let a Nicol prism[*] be placed so that its axis is in a line with the axis of the eye, with the longer diagonal of the lozenge-shaped field vertical, and so that the oblique face of the spar retreats from the eye from left to right. If then the eye be moved so as to explore the visible interior, it will be seen that the light of the "field" (which, it will be observed, is that of the ray now usually called the extraordinary ray) is terminated to the left by a margin of violet-blue light running vertically in almost a straight line, having the violet edge to the left, next the darkness, and the blue and bluish-green portion to the right, next the light. Further, this field is terminated to the right, not by the "orange band" and fringes, but simply by the limits of the edges of the prism.

3. The "orange band" is situated at the termination of a second "field" of less intense illumination, which only comes into view when the light passes to the eye obliquely through the prism. This is the field of the "ordinary" ray; as may be proved by first passing the light directly down the axis of a second Nicol prism also having its longer diagonal vertical, when the second field, together with the orange band and fringes, disappears totally. The "orange" band consists of red, orange, and orange-yellow rays, the former lying more to the left, the latter more to the right. The fringes, which, with an important modification, present the colours of Newton's transmitted rings, lie right within the coloured band, and extend beyond it parallel to its edge. They are curved in form, the sinus of the curve being toward the right. The visible curvature is greater the nearer the eye is brought toward the prism. Their form approximates to that of the arc of a circle; though really only a portion of a conchoid curve which becomes at last asymptotal to a line varying with the position of the eye. Could the eye be actually plunged within the substance of the Nicol prism, the curves would, as will hereafter be indicated, appear truly circular in form.

---

[*] My experiments have been made chiefly with two Nicol prisms, by Tisley and Spiller, of 1 inch clear aperture each, and with a larger prism of 1¾ inch aperture; but the phenomena may be observed with any Nicol prism whatever.

4. When monochromatic light is employed, the fringes in the "orange band" become more numerous and sharper; and a second set of fringes, not otherwise noticeable, appears within the edge of the "blue band:" these fringes are precisely similar in kind with those of the "orange band," but their curvature is usually so slight as to be unobserved. If a thin prism of glass of suitable angle be interposed in the path of the ordinary ray, the particular dispersion of the "orange band" may be corrected, and the fringes are then left sharper and brighter; the second bright band of the fringes is the brighter. The glass prism may be employed either between the eye and the Nicol prism, or beyond the Nicol prism, in this experiment. The particular dispersion of rays that produces the "blue band" may also be corrected by a glass prism, but only when this is placed between the eye and the Nicol prism, not when beyond the Nicol. When the blue band is thus eliminated, the fringes observed in monochromatic light may be seen at the termination of the field of the extraordinary ray to the left. The fringes of both sets appear narrower in blue light than in red light.

5. The light which gives the extreme red portion of the "orange band" meets the first surface of the Nicol prism at an angle of about 18° 15′ with the axis of the prism, passes through the prism almost parallel to its axis, and emerges parallel to its former path. It, and in fact all the rays of the second "field" (that consisting of ordinary rays, and seen obliquely), suffer therefore a lateral displacement. The light of the extreme violet ray of the "blue band" meets the first surface of the prism at an angle of about 80°, and it and most of the rays of the bright "field" (that seen directly, and consisting of "extraordinary" rays) pass almost in straight lines through the prism, and emerge without suffering deviation or any considerable displacement. The extreme violet ray makes an angle of about 9° with the axis of the prism.

6. If the light of the "blue band" be received upon the horizontal slit of a spectroscope, the presence of dark interference-stripes reveals the character of the light of that region of the field.

Cause of the Phenomena.

7. The two sets of fringes—those of the "orange band" and those of the "blue band"—are due to interference taking place within the film of "balsam" at the critical angle of total reflexion for ordinary and extraordinary rays respectively. The production of interference-phenomena in a thin film placed beneath a prism of a more highly refractive substance, and occurring just within the limit of total internal reflexion of the prism, was first observed by Sir W. Herschel, and is described in detail by Sir John Herschel in his 'Essay on Light,' art. 641. It is there shown that, to an eye immersed in the prism, an iris is formed at the edges of the cone of rays which reach the eye at the critical angle, and that this iris is blue for reflected, red-orange for transmitted rays; the separation of the colour being the simple consequence of the total reflexion following the index of refraction, which, being higher for more refrangible rays, causes them to meet the eye in a cone of lesser angle. The fringes seen within the red-orange iris that thus borders the limiting circle of transmitted rays follow naturally from a consideration of the paths travelled by the various rays in their passage through the thin film below. They follow the order of Newton's rings, except that those nearest to the limit are deprived of the rays of higher refrangibility within whose critical angle they lie; but the regularity of their form and tint is disturbed, to an eye outside the prism—the cone being distorted and the circularity of the arcs destroyed, and the fringes being displaced and confused by dispersion at the outer surface of the prism.

This experiment of Herschel's is usually made with an equiangular prism and a piece of plate glass pressed against its base. The width of the fringes increases with pressure as the thickness of the film of air diminishes. As the critical angle depends on the index of refraction out of the film into the prism, the cone of rays has a wider base when a film of water is used instead of a film of air; and the nearer the index of the film to that of the prism, the greater becomes the radius of curvature of the iris and fringes.

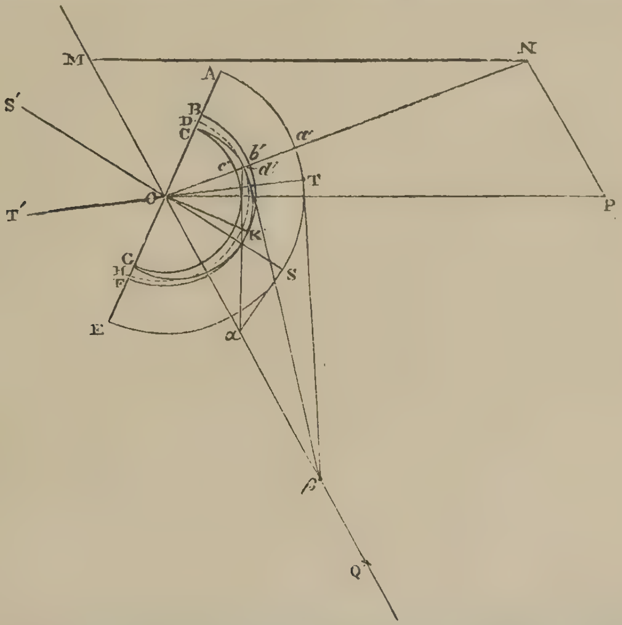
Of the Conditions of Total Reflexion.

8. In order to apply the case of the iris and fringes observed

by Herschel to the phenomena of the Nicol prism, it will be necessary next to prove that, at certain angles of incidence, both the ordinary and extraordinary ray suffer total reflexion at the surface of the separating film.

Most text-books assert, on the contrary, that total reflexion of the ordinary ray does not occur*. The original paper of Nicol spoke only of *increasing the divergency between the two rays*; and it was left to the late Mr. H. F. Talbot to point out† that both rays came through at a certain obliquity, but that one image vanishes by total reflexion *before* the other.

9. The following modification of the construction of Huygens will enable us to establish the correctness of Mr. Talbot's statement. Let the parallelogram M N P O represent a principal section through the axis of the Nicol prism, O and M



---

* *Vide* Tyndall, 'On Light,' p. 125:—"Now the refractive index of Iceland spar is for the extraordinary ray less, and for the ordinary ray greater than for Canada balsam. Hence, in passing from the spar to the balsam, the extraordinary ray passes from a less refracting to a more refracting medium, where total reflection cannot occur;" &c.

† "Facts relating to Optical Science, No. II. On Mr. Nicol's Polarizing Eyepiece," Phil. Mag. 1834, p. 289.

being the two ends of the shorter diagonal of the lozenge-shaped face. O N represents the trace upon this plane of the film of balsam, making an angle of about 20° 40′ with O P. Produce O M to Q. Through O draw a line A O E parallel to the direction of the optic axis of the crystal, making an angle of about 47° with O M. About O as centre describe three semicircles having radii O A, O B, O C, respectively proportional to the velocity of light in air and in the extraordinary and ordinary rays of calc-spar. Draw another semicircle with radius O D proportional, on the same scale, to the velocity of light in Canada balsam. The following Table gives these values (air = 1), together with the corresponding indices of refraction for red and violet rays (rays B and H of Fraunhofer):—

		Index of refraction.	Velocity (air = 1).
Ordinary ray......	Red....	1·65308	·60493
	Violet..	1·68330	·59407
Extraordinary ray...	Red....	1·48391	·67386
	Violet..	1·49780	·66765
Canada balsam....	Red....	1·529	·654
	Violet..	1·553	·644

These circles cut O N in a', b', c', and d' respectively. Draw O K perpendicular to A E, and construct the semi-ellipse C K G. This represents the section of the ellipsoid of revolution for the principal section of the crystal in the plane of the diagram. The direction of the rays that pass at the critical angles may then be determined as follows:—For the ordinary ray, from the point d' draw a tangent to the circle C c' G, and produce it till it meets O Q in α; from α draw α S a tangent to the circle A S E, and produce S O to S′. All rays meeting the face M O at angles less than M O S′, by ordinary refraction will traverse the film O N, while all incident at greater angles will be totally reflected. For the extraordinary ray, draw from d' a tangent to the semi-ellipse C K G, meeting O Q in β; draw β T a tangent to the circle A S E, join T O, and produce to T′. All rays incident on the face O M, within the angle M O T′, and entering by extraordinary refraction, will be transmitted, but those incident at greater angles will be totally reflected. Hence the true "field" of the Nicol prism, in which only the extraordinary rays are found, is that of rays incident within the angle S′ O T′. Supposing that we have hitherto taken the indices of refraction for red rays in our construction, these angles will determine the limits of transmission for red rays. If now new circles be drawn* having the values of O B, O C, and O D to correspond to violet rays, and a new semi-ellipse be drawn as before, the same construction will be found to give slightly different positions for the lines O S′ and O T′. For violet the limiting angle for ordinary rays is rather less than M O S′, and for extraordinary rays rather greater than M O T † (not shown in small diagram).

10. It is well known that the refractive index varies in dif-

---

* This has not been attempted in the diagram, as an inconveniently large scale would have to be chosen, and then the construction would be confusing without the use of coloured lines.

† It can readily be shown, on simple algebraical reasoning, from the data given, that the circle D d' H cuts the line O N outside the ellipse. Call r the portion of line O N intercepted by the ellipse, and call angle A O N, θ. This angle never exceeds 48° 25′, and is less in some prisms. Taking x and y as the rectangular coordinates of the intersecting point, when the ellipse is referred to O as origin and to principal axes as coordinates,

$$x = r \sin\theta \quad\text{and}\quad y = r\cos\theta.$$

Substituting these values in general equation to ellipse with origin at centre, we get

$$\frac{r^2 \sin^2\theta}{a^2} + \frac{r^2\cos^2\theta}{b^2} = 1,$$

and

$$\frac{b^2\sin^2\theta}{a^2b^2} + \frac{a^2\cos^2\theta}{a^2b^2} = \frac{1}{r^2},$$

which may be reduced to the form

$$r = \frac{a}{\sqrt{\sin^2\theta + \frac{a^2}{b^2}\cos^2\theta}}.$$

Inserting the appropriate values for red and for violet rays respectively, as given in the Table in § 9, the values of r are found to be 0·6369 and 0·6247, which are severally less than the respectively corresponding values of the radius O d', 0·654 and 0·644. In either case, therefore, a tangent to the ellipse can be drawn from d', that is to say, it is possible for total reflexion to occur to each ray; and there is consequently in the Nicol prism an obliquity of incidence at which both rays are transmitted, another obliquity at which, while the extraordinary is transmitted, the ordinary ray is reflected aside, and another at which both rays are totally reflected.

ferent specimens of Canada balsam. If one with too high a refractive index be taken, O D will approach O C in value, and then it may happen that d' will fall within the ellipsoid; in which case it will be impossible to draw a tangent, the corresponding fact being the impossibility of total reflexion at any angle. After carefully working this construction for the best determinations of the various refractive indices by Rudberg, Wollaston, and Brewster, the author has been unable to discover such a case. As, however, it is notorious that in the large Nicol prisms now manufactured ordinary Canada balsam cannot be employed, but a harder gum-resin, such a case may occur; but then there will be no "blue band" unless the point d' still falls without the ellipsoid for red rays.

Of the Red-orange Iris.

11. From the foregoing considerations it will now be apparent why a *red-orange* iris is seen at the limiting surface of the ordinary rays, for it has been shown that the limit for rays of high refrangibility lies within the angle M O S′; hence red and orange rays are transmitted at angles of incidence at which no blue or violet or green can pass.

Of the Blue Iris.

12. It will also be seen that if the limit for violet extraordinary rays lie beyond M O T′, violet and blue rays can be transmitted at angles at which red and orange rays are cut off. Hence the iris at the limiting angle will be blue, not orange. It will not be forgotten that O T is incident at about 80° to O M, at which angle the ray would suffer little deviation and dispersion; but this deviation and dispersion is in reality from, not towards, the normal to the surface, since the crystal is negative and the ray extraordinary; so that the dispersion is reversed for rays entering at angle M O T′.

A simple experimental proof of this reversal of dispersion is obtained by looking through a Nicol prism at a narrow opaque body against the light, when slight tinges of blue and of yellow appear at their edges; but the positions of these blue and yellow margins are reversed in the two fields.

The blue iris is not so much curved as the orange iris, since the index of the balsam of the film is nearer to that of the extraordinary ray than of the ordinary ray. For the same reason the cone of rays coming to the eye must be more obtuse, and hence, for the same thickness of film, the interference-fringes must be nearer together and finer; they will therefore be the more easily obliterated in the subsequent dispersion at the face of the prism nearest the eye, as in fact is the case, except when the dispersion is corrected by a thin glass prism, or avoided by the use of monochromatic light.

University College, Bristol,
May 21, 1877.

Note.—In Foucault's prism, where both rays are totally reflected at the surface of a film of air, instead of balsam, the angular magnitudes of the limiting cones of transmitted rays are much less, and the field is consequently narrower. Moreover, as the extraordinary ray in this case suffers total reflexion when the limiting angle of incidence is much greater than in the Nicol prism, the deviation and dispersion are toward the normal to the surface, and there can be no blue iris. In the Foucault prism, in fact, a series of coloured bands and fringes exists at the limit of each of the two fields, but each is red-orange.—S. P. T.

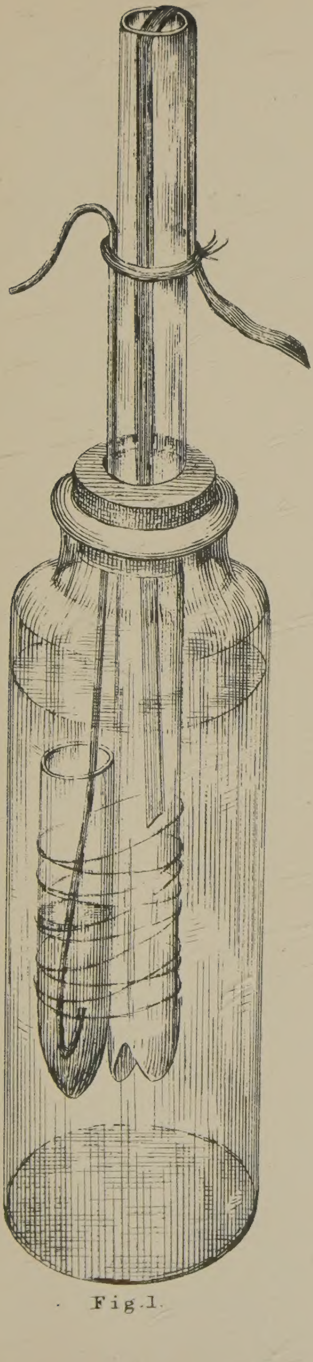


Fig. 1.

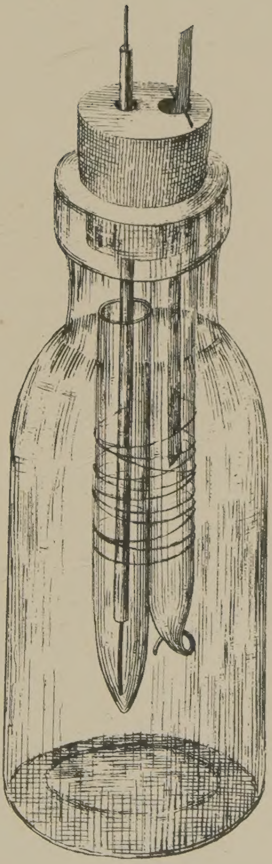


Fig. 2.